Price and Information in Life Microinsurance Demand:

Experimental Evidence from Mexico

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Abstract

Poor households in developing countries face large and varied risks, but often have inadequate informal tools to manage them. Microinsurance is being developed to create a better alternative, and it should--in theory--be in high demand. Yet take-up of microinsurance remains low. I study the impact of price and information on the demand for life microinsurance among microfinance borrowers of Compartamos in Mexico. I randomly assigned 8,700 borrowers to two of four treatments: (i) no longer receive a base amount of subsidized insurance coverage (high price) or keep the subsidy (low price), and (ii) being informed with a message emphasizing the financial toll of a funeral and how the insurance payoff helps to face it (financial information) or information emphasizing the emotional toll of a funeral on the surviving family (emotional information). On average, eliminating the subsidy led to a decrease in insurance coverage, but the two messages did not impact coverage. The impacts are heterogeneous, however. Although all borrowers decreased their coverage as the subsidy was eliminated, younger borrowers presented with the emotional information were less likely to drop coverage than their counterparts presented with the financial information. The impact was reversed for middle-aged borrowers: the financial information led to a smaller drop in coverage following the elimination of the subsidy. The findings add to the literature on how information drives behavior in developing countries, and suggest that specific information provided at the time of choice was critical to help borrowers make a decision regarding a risk management strategy.

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1. Introduction

Poor households in developing countries face large and varied risks. Many agriculture-dependent households, for example, are at risk of drought- or flood-induced crop failures or livestock deaths. A household's income can plummet if its highest earner becomes ill and cannot work, even for illnesses that are cheaply preventable or easily treatable. The death of a family member often implies having to fund expensive burial ceremonies for which households try to save for a long period of time yet often end up paying for by borrowing large sums at high interest rates. If the deceased was the household's primary earner, replacing her/his stream of income is an even bigger problem.

A major push is currently underway to expand access to formal insurance products to help poor households in developing countries weather shocks and deal with irregular incomes. Microinsurance—insurance targeted to the poor through low premiums and/or low coverage limits—includes products to protect households against risks such as too little or too much rainfall, health events caused by sickness and/or accidents, death, or property loss or theft.

Microinsurance should in theory be in strong demand to act as a safety net for poor families who rely mostly on self-insurance, traditional risk-sharing arrangements, informal insurance networks, and credit and savings to deal with shocks. Take-up of microinsurance products, however, remains low overall. For example, Matul, McCord, Phily, and Harms (2010) find that 2.6 percent of the African population living with less than $2 per day uses microinsurance. Other studies, described below, also show low rates of take-up.

This paper studies demand for a life insurance product offered to borrowers of Compartamos, a large microfinance institution in Mexico. Funerals are expensive events. In Mexico, a short survey of 15 Compartamos borrowers who experienced a death in their household indicated that they spent on average 25,300 Mexican Pesos for a funeral, or almost six months of these households' average income. Funeral and life insurance products exist, both formal and informal ones, but provide limited coverage. For instance, even though 80 percent of South African households surveyed by Collins and Leibbrandt (2007) carried at least one form of funeral insurance, 61 percent remained underinsured.

The microinsurance product studied here is a term life insurance policy. The policy covers individuals for 19 weeks, or about the duration of a loan cycle. The insurance is sold in modules, providing coverage of about US$1,175 each at a cost of about US$4.50 per module. Prior to the experiment, Compartamos subsidized insurance coverage by providing one module to all borrowers at no cost to them; Compartamos paid the premium for this module directly to the insurer who underwrote all policies. In the microinsurance context, this type of subsidy provides an important advantage: it offers borrowers a first experience with the product. Given that issues of trust in the insurance provider and familiarity with insurance are key determinants of low microinsurance take-up (Cai, Chen, Fang, & Zhou, 2009; Cole et al., 2012; Dercon, Gunning, & Zeitlin, 2011; Giné, Townsend, & Vickery, 2008), this coverage subsidy may be a particularly effective way of increasing participation in microinsurance schemes. In addition to the subsidized module, borrowers had the option to purchase up to 7 additional modules, at a cost of about US$4.50 each, for a total possible coverage of $9,400. The fact that 52 percent of all of Compartamos' borrowers purchased one or more modules shows that borrowers understand and value the product. In the analysis sample, this take-up rate was 69 percent pre-experiment.

To measure the influence of price and information on the demand for life microinsurance, I randomly assigned group of microfinance borrowers to one of four treatments constituted by two manipulations. First, the subsidy offered to borrowers in the form of a free module of insurance coverage was eliminated in some groups of borrowers ("village banks") and not others. Eliminating the subsidy led to an increase in the price of coverage. Almost all borrowers who purchased additional modules purchased only one module, so eliminating the subsidy implied a 100 percent increase in the price of coverage (assuming that borrowers keep the same amount of coverage). Note that the elimination of the subsidy produces a discrete change in the price of coverage, but also implies that borrowers in the subsidy treatment group lose the benefit of being automatically covered by the policy. [1] The second treatment manipulated the information given to borrowers about the insurance. Some village banks were presented with information emphasizing the financial toll of a funeral and how the insurance payoff helps to face it, while others received information emphasizing the emotional toll of a funeral on the surviving family.

The results indicate that, on average, borrowers were sensitive to an increase in the price of coverage. Eliminating the subsidy led to an 11-percentage point decrease in the likelihood that borrowers have any insurance coverage and a 0.86-module decrease in the number of modules of coverage. On the other hand, the information given to borrowers did not have any impact on coverage on average.

This average, however, hides important heterogeneity. The information treatment, and the age of the borrowers, interacted with the impact of eliminating the subsidy. Information had opposite effects on younger borrowers and middle-age borrowers. Younger borrowers (aged 17 to 29) were more likely to have some insurance coverage when they were presented with the emotional message rather than the financial message. Middle-aged borrowers (aged 30 to 49), in contrast, were more likely to have some coverage when presented with the financial information

[1] The endowment effect implies that the impact of offering the subsidy on insurance coverage would be different from the impact of eliminating the subsidy, which is measured here.

rather than the emotional message. Finally, while borrowers aged 50 years and above decreased their coverage less than those aged 49 and below when the subsidy was eliminated, they did not do so differently when different information was given to them.

The findings from this experiment suggest that information is a key determinant of the risk mitigation decisions of poor households in developing countries. Informing individuals in developing countries has been shown to help them make better decisions regarding their health, from which well to draw water from (Bennear et al., 2012) to what sexual behavior to avoid in order to reduce the risk of contracting HIV (Dupas, 2011). Death is a low-probability but high-expenditure event, making it hard to plan effectively for it. Raising the salience of the risk may not be enough, in itself, to trigger the adoption of new –and improved– strategies to manage that risk, but providing specific information at the time of decision could help households make choices that help increase their risk protection and welfare.

2. Background

2.1. Microinsurance and informal insurance mechanisms

Formal insurance mechanisms do not operate in a vacuum but complement existing informal insurance systems. Poor households diversify their activities to deal with risk *ex ante* and use social networks, sale of assets, credit, savings and other tools to cope with negative shocks *ex post*. Previous literature has tried to establish whether these informal mechanisms provide full insurance in an efficient manner.[2] An important paper by Townsend (1994) provides a general equilibrium test of the full insurance model at the village level: if community-level

[2] *Ex ante* mechanisms are not reviewed here because they differ from the insurance product studied. See for example Morduch (1995).

informal mechanisms provide good protection against adverse shocks, household consumption should not vary importantly when the household experiences an idiosyncratic shock, controlling for village aggregate consumption. Using detailed data from three poor villages in southern India, Townsend is not able to statistically confirm the full insurance model, but concludes that his results are "surprisingly" close. If members of a village are able to effectively partially insure each other against idiosyncratic shocks, this could explain why demand for formal microinsurance products remain low, and suggests that formal microinsurance may not have a large net beneficial impact.

Later work, however, largely questions this conclusion. Kazianga and Udry (2006), for example, study rural areas of Burkina-Faso at a time when a significant drought affected the income of agriculture- and livestock-dependent households. The aggregate shock provided by the drought was accompanied by "substantial idiosyncratic income variation" due to differences in soil types by farming plot. The authors find evidence for some level of food consumption smoothing, but one that does not prevent significant drops in household food consumption matching the idiosyncratic drops in household income (conditional on aggregate shock). Another test of the full insurance model can be conducted within households, who can be expected to exhibit a higher degree of solidarity than a village. Duflo and Udry (2004) show that little risk sharing happens even between household members. They analyze whether rainfall shocks affecting the plots and crops grown by men or women in Ivory Coast produce similar changes in household consumption. Since men and women within a household farm different plots with crops that are differently susceptible to rainfall, a rainfall shock provides an exogenous variation in income within the household. They find that, conditional on overall expenditures, a shock to women's income increases food expenditures while a shock to men's income reduces food expenditures but not expenditures on private goods such as tobacco and alcohol. More recently, Robinson (2012) used a randomized experiment to create an exogenous shock to a household, and rejects the hypothesis of intra-household efficiency. In other words, women and men within the household fail to perfectly insure each other against shocks.

On balance, the evidence on risk sharing in poor communities and poor households points to the existence of a clear space for formal insurance to improve households' and individuals' protection against risk. Informal mechanisms exist but fail to completely smooth consumption (Collins & Leibbrandt, 2007), so that expectations of a large demand for microinsurance are not misplaced.

2.2. The take-up of microinsurance products

Given the lack of adequate protection that poor households enjoy, the take-up rates of microinsurance products are puzzlingly low. Cole et al. (2012), for example, study demand for a carefully-designed and actuarially fairly-priced rainfall insurance product in the Indian states of Gujarat and Andhra Pradesh, where households cited droughts as the most significant risk they face. Yet take-up of the insurance was only between 24 and 29 percent when households were exposed to various treatments designed to raise their awareness and understanding of the product, and "close to zero" for households in the same villages who did not participate in the study. Karlan, Morduch, and Mullainathan (2010) reported take-up rates between 20 and 47% for two health microinsurance products in the Philippines, when marketing was conducted door-to-door at the home of potential clients. For funeral products, take-up remains similarly low. Giesbert, Steiner, and Bendig (2011), for example, surveyed 350 households in central Ghana, and found that only 4.5 percent of them held a formal life microinsurance policy.

A growing literature tests the contribution of various factors to the low demand for several microinsurance products. As assumed in Giné et al. (2008)'s neoclassical benchmark model, price, wealth and risk aversion have a large impact on take-up. Cole et al. (2012) estimated the price elasticity of demand for a rainfall insurance product in India between -1.04 and -1.16. To my knowledge, no other study estimates price elasticities, but offering subsidies and vouchers was found to significantly increase demand for several microinsurance products in different settings (Dercon et al., 2011; Giné, Karlan, & Ngatia, 2011; Thornton et al., 2010). Wealthier households were also found more likely to purchase life microinsurance in Ghana (Giesbert et al., 2011) and rainfall microinsurance in India (Giné et al., 2008) and Malawi (Giné & Yang, 2009). Running counter to the benchmark model, risk aversion is negatively associated with microinsurance take-up (Giesbert et al., 2011; Giné et al., 2008), a fact that (Dercon et al., 2011) attribute to the lack of trust that individuals have in the ability of the insurer to honor payments when claims are made. I return to the importance of trust below.

Several extensions of the benchmark model developed by Giné et al. (2008) have a large influence on microinsurance demand. Credit and liquidity constraints have been established are one of the most important non-price frictions that limit demand. Cole et al. (2012), for example, report that poor households in Andhra Pradesh and Gujarat cite "insufficient funds to buy insurance" as the main reason for not purchasing insurance. Providing cash grants of Rs100 vs. Rs25 (Rs100 provided enough cash to purchase a rainfall microinsurance policy) increased take-up by about 40 percentage points, more than doubling the average take-up rate.

Other determinants of take-up have been receiving a lot of attention and supporting evidence. One of them is trust in the insurance provider. Cai et al. (2009) show how, although farmers are not fully insured through informal mechanisms, a lack of trust reduced demand for formal microinsurance for sows. Giné et al. (2008) and Cole et al. (2012) also provide experimental evidence that trust hampers demand for rainfall insurance in India. Dercon et al. (2011) developed and tested a model of the demand for a composite health and funeral insurance product that explains how the level of trust in the provider impacts take-up overall and interacts with price sensitivity and risk aversion.

Finally, other determinants found to impact take-up of microinsurance include understanding of insurance and financial literacy (Gaurav, Cole, & Tobacman, 2011), salience and framing cues (Cole et al., 2012), social networks and peer effects (Dercon et al., 2011; Giné et al., 2011), and convenience of enrollment (Thornton et al., 2010).

The model of Giné et al. (2008) was developed for rainfall microinsurance, but Giesbert et al. (2011) test it for life microinsurance. They analyze correlates of demand for life microinsurance in two regions of Ghana with a cross-sectional survey of 350 households. Like Giné et al. (2008), they find suggestive evidence for deviations from the neoclassical model, particularly the importance of trust in the insurance provider and the role of social networks in driving demand. Their study, however, is not able to establish causality, and the paper ends with a call for further work on the take-up of less-studied microinsurance products, such as life policies, with randomized experiments.

The impact of subsidies on microinsurance take-up has mostly been studied through their influence on price at the time of offering the product. One study, however, measured the impact of eliminating a subsidy. Thornton et al. (2010) investigated the demand for health microinsurance among informal workers in Nicaragua by manipulating the costs and convenience of enrollment. A subsidy given in the form of a voucher for an amount equivalent to 6 months of coverage had a large impact on take-up. When about 20 percent of the sample signed up for coverage, those assigned to receive the voucher were between 28 and 33 percentage points more likely to purchase the insurance. One year later and without subsidy, however, less than 10 percent of those individuals remained in the program.

3. Field setup and Data

3.1. Field setup

This research was conducted in partnership with Compartamos, a large microfinance institution in Mexico. The interventions were implemented in the state of Sonora in northern Mexico. Compartamos offers loans under joint-liability contracts with group lending, as well as individual loans. The present research, however, is based solely on clients borrowing under the "village bank" group methodology. All of them are women. Groups vary in size between 10 and 50 borrowers, with an average of 19.

The life insurance policy is a term policy that lasts 19 weeks, almost concurrent with a loan cycle.[3] Compartamos acts as an intermediary between the insured (its borrowers) and a large private insurer: it markets the insurance, collects premiums for the insurer, and receives claims. Policies are available to all active borrowers[4] regardless of age and medical condition (no medical certificate is asked), and cover natural and accidental death of the borrower only.[5] Insurance is sold in modules of 15,000 Mexican Pesos (about US$1,175 at the time of the experiment) of coverage, for a premium of 57 Pesos (about US$4.50) per module. A survey of

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[3] Loan cycles last 16 weeks. If a client takes a new loan at the next loan cycle, a new policy comes into effect at the time of the new loan disbursement and cancels the previous policy. If a client chooses not to borrow at the next loan cycle, the policy remains in effect for three weeks after the end of the last loan cycle.
[4] This specific policy is only available to individuals who borrow from Compartamos, although the insurer offers similar policies to the general population.
[5] The insurance does not cover death because of suicide or illegal action of the insured or beneficiary. These cases are rare.

borrowers indicated that total funeral costs in this population range between 25,000 and 50,000 Pesos, with most households spending an amount at the lower end of that range. Prior to the experiment, all borrowers in the group lending methodology benefited from one module of coverage at no cost to them, paid for by Compartamos, and were able to buy additional modules of coverage. All borrowers therefore had experience with the product at the time of the experiment. New borrowers and borrowers aged 70 years and older are limited to purchasing one additional module, while other clients can buy up to seven additional modules for a total coverage of 120,000 Pesos (about US$9,400). In practice, 99.3 percent of borrowers who purchase insurance only purchase one module.

To make the policy easier to understand and more attractive to low-income borrowers, paperwork is reduced to a minimum, both at the times of paying the premium and claiming benefits. Signing up consists of paying the premium and providing a photocopy of the beneficiary's official identification document. In case of a claim, the payoff is disbursed an average of two days after a copy of the death certificate has been provided by the beneficiary (on average eight days after the death of the insured has been notified to Compartamos). The death certificate, a copy of the beneficiary's identification card and the insurance certificate are the only required documents for a claim; in practice, the insurance certificate is kept in Compartamos' office. Claims are paid even if the borrower was in default on her loan at the time of her death (outstanding loan balances are automatically forgiven if a borrower dies).

3.2. Data sources

Data for the analyses were obtained from two internal databases of Compartamos: the loan database provided a limited set of indicators about the borrower and her household, and the insurance database provided information about insurance purchases and claims. They were matched with a unique borrower identification number. Because no field survey was conducted, the number and type of indicators exploitable in the analysis are limited to those that Compartamos captures as part of its loan application and insurance purchase processes. In addition, since loans are the joint responsibility of the borrowers in a group, Compartamos does not need full information on its borrowers to underwrite loans and gathers a limited information set on its clients. Notable indicators that are not available are borrowers' income, and any business data. The loan database provides limited data on the borrower, her household and her credit history with Compartamos. Demographic or household indicators include age, marital status, education level, number of children, number of "economic dependents" and home ownership status. Credit history data include the name of the borrower's group, the number of loan cycles of the group and of the borrower (borrowers are allowed to switch group and join an existing or a new group) and the borrower's current loan size. If a borrower leaves her group and joins another one, the name of the group she left and the reason for the switch are also available. Finally, the insurance database includes indicators such as the number of modules of additional insurance purchased and, if a claim was filed, the dates of death and claim filing, age of the borrower at death, amount of the payoff, and cause of death.

4. Experimental design and Empirical strategy

4.1. Design of the experiment

In order to measure the causal impact of the subsidy and information on the demand for life insurance, groups of borrowers were randomly assigned to two of four groups following a 2-by-2 randomization design.

In the subsidy treatment group, the subsidized insurance coverage was eliminated; borrowers did not benefit from any free coverage and needed to purchase one or more modules if they wished to have any coverage. Borrowers in the subsidy control group continued to avail of one free module of insurance, as well as the possibility to buy additional modules. All other characteristics of the insurance, such as the price and payout of each module, sign-up and claim processes and requirements, and eligibility criteria remained unchanged. The no-subsidy treatment changed the price of coverage, to an extent that is a function of the amount of insurance that borrowers bought before the experiment. Table 1 shows the breakdown of cost increases for all levels of coverage. For clients who purchase one module of insurance beyond the subsidized module (almost all clients who buy additional insurance), the no-subsidy treatment effectively doubled the average price of coverage; for clients who purchase all seven modules (0.1 percent of clients who buy additional insurance), the average price of the insurance increased by 17 percent, assuming that they continue to purchase the same number of modules of insurance.

Table 1. Change in the price of insurance coverage created by the subsidy treatment.

	Total coverage (pesos)							
	15,000	30,000	45,000	60,000	75,000	90,000	105,000	120,000
With subsidy (control group)	0	57	114	171	228	285	342	399
Without subsidy (treatment group)	57	114	171	228	285	342	399	n/a
Increase	-	*100%*	*50%*	*33%*	*25%*	*20%*	*17%*	-

Independently from the elimination of the subsidy, borrowers were assigned to receive one of two sets of information. The information set included a poster, displayed conspicuously during the group meeting, and a script that loan officers used to explain the poster. Posters are reproduced in Appendix 1 and Appendix 2. The top half of both posters was identical, and presented five key characteristics of the product. The bottom half of each poster, and the script accompanying each poster, was tailored to provide two different messages.

In the “information treatment group,” borrowers were informed about the product and its benefits with a message emphasizing the financial toll of a funeral and how the insurance payoff helps to face it. The financial poster displayed the costs associated with the death a family members and how a hypothetical borrower faces these costs without and with insurance. Figures were calculated from actual experiences of Compartamos borrowers, which were collected in a short survey.

In the “information control group,” borrowers were informed about the product and its benefits using an emotional poster, emphasizing the emotional toll of a funeral on the surviving family. The poster displayed a cartoon depicting the story of a borrower who purchased insurance, passed away and whose family enjoyed the benefits of the payout. The emotional information set is considered as an information control group because it codified Compartamos’ previous marketing approach; this marketing by example had been reported by loan officers are their most common product information and promotion strategy.

The posters and the scripts were provided to loan officers to support their presentation of the product, standardize the information provided to all borrowers, and increase the salience of the risk in a comparable way in both information groups. Having the information control group receive a poster rather than the previous marketing approach prevents me from comparing the impact of the new approach to that of the previous one, but allows me to compare the impact of the financial vs. emotional information, at similar levels of risk salience.

The news about the elimination of the subsidy (where applicable) and the new information were given to borrowers by loan officers during a village bank meeting, according to the following timeline. In week 15 of the 16-week cycle, borrowers inform loan officers of the loan amount they desire for the next cycle, and whether they wish to purchase additional modules of insurance. The news that the subsidy would be eliminated was given at that time, along with the new information, prior to the beginning of the insurance sign-up process. The experiment was implemented in 567 groups that reached their week 15 between February 1st and March 16th, 2012. I measured the impact of each manipulation on insurance take-up and coverage in the following cycle.

Randomly assigning borrowers to each treatment is particularly important in this study for at least two reasons. First, selection biases are particularly acute in microfinance, where unobservable characteristics of individuals are likely to be important determinants of participation, since individuals who are more entrepreneurial or motivated are more likely to avail of microfinance products and services as well as to score higher marks on socio-economic indicators. Second, eliminating endogeneity is particularly important in this analysis since no baseline survey was conducted and the number of variables available to statistically control for confounding variables is limited. Risk preferences among clients, for example, are likely to be important determinants of the demand for life insurance but are not available in Compartamos' administrative data.

The random assignment was done at the level of the group, and stratified by loan officer. While assigning groups rather than individuals to each treatment requires a larger sample size, individual-level randomization was infeasible in this setting. The promotion and sale of the insurance is done during group meetings, with all members in attendance (a fine is imposed for not coming to the weekly group meetings). The stratification by loan officer implies that all loan officers manage village banks in all treatments. This approach is particularly justified by the fact that loan officers conduct the village bank meetings, imprinting on all their groups some of their own personal style and characteristics. Even though randomization eliminates bias due to self-selection in expectation, in practice it is possible that a given randomization produce an unbalanced assignment. The stratification guarantees that the characteristics of the loan officers and of their relationship to the borrowers in the groups they manage was orthogonal to the outcome of interest. In practice, the assignment of village banks to loan officers is frequently changed, based on staff turnover and with the objective of preventing fraud. The stratification by loan officer was therefore largely undone since loan officer reassignment occurred during the experiment. In a few cases, almost all the village banks of some loan officers were assigned to one treatment. The benefits of the randomization remain despite the lack of stratification (I show checks on the randomization in Table 2), and I include a full set of loan officer binary variables in all regressions to address potential biases.

4.2. Empirical strategy

The econometric analysis of random assignments is typically straightforward. If the randomization is successful, a simple comparison of means indicates the impact of the manipulation implemented on the outcome of interest. To account for the levels of randomization and analysis, and to take advantage of historical data on existing borrowers, I analyze the impact of the elimination of the subsidy on each borrower's decision's to purchase insurance or not in a fixed-effects panel regression framework where the unit of observation is the borrower. The regression model used to analyze the impact of the treatments is

$$C_{ig} = \alpha + \beta T_g + \delta P_{ig} + \rho(T_g * P_{ig}) + \theta X_{ig} + u_g + \varepsilon_{ig}, \quad (1)$$

where C_{ig} measures insurance coverage as detailed in the next paragraph, T_g is a variable indicating the village bank's treatment status as detailed below, P_{ig} is a binary variable equal to 1 for the loan cycle after the intervention was implemented, X_{ig} is a binary variable indicating whether the borrower purchased insurance in her last loan cycle before the experiment, and u_i denotes a vector of borrowing group, loan officer and time fixed effects. Time fixed effects are included as a series of binary variables controlling for the month when the loan cycle starts, since insurance purchases vary along the year. Standard errors were clustered at the village bank level. The treatment dummies were coded based on the assignment, for an intent-to-treat analysis.[6] The subsidy treatment was coded as a binary variable equal to 1 if a borrower's group was assigned to lose the benefit from the subsidy and 0 if her group was assigned to keep the subsidy. The information treatment was coded as a binary variable equal to 1 if a borrower's group was assigned to be presented the financial information and 0 if her group was assigned to receive the emotional information.

I measure the impact of the subsidy and information treatments on two indicators of insurance coverage, denoted as C_{ig} in equation (1) above. The first is a binary variable that takes the value 1 if borrower *i* in group *g* is covered by any module of insurance (including the subsidized module, so that at baseline this indicator is 1 for all borrowers). This analysis uses linear probability models to avoid the incidental parameter problem that affects non-linear regressions with panel data, particularly when the number of periods of data is low (Greene,

[6] Since borrowers are allowed to change group between loan cycles, some borrowers assigned to a particular treatment (e.g., not receiving the subsidy) may take their next loan in a village bank that was assigned to another treatment (e.g., keeping the subsidy). I investigate possible biases on my results in section 4.3.1.

2008). The second indicator on the left-hand side of equation (1) is a categorical variable indicating the number of modules of insurance coverage that borrower *i* in group *g* has (range: 0 to 8).

4.3. Design checks

In this section, I verify the integrity of the study design. I show that the randomization of village banks was successful, attrition is uncorrelated with the treatment assignment, the implementation of the treatments did not compromise the design, and spillovers were unlikely to have happened.

4.3.1. Randomization results

While randomization produces identical groups in expectation, the characteristics of treatment and control groups may be different in practice. Table 2 reports the results of the randomization of groups into each of the four treatments. It shows that the group-level randomization was successful, but that differences exist at the level of individual clients.

Table 2. Randomization balance.

	Subsidy treatment			Information treatment		
	Subsidy	No subsidy	p-value	Emotional message	Financial message	p-value
Panel A. Group-level indicators						
Take-up of additional modules (%)	66.1	64.5	0.531	65.3	65.4	0.973
Number of modules of coverage	1.70	1.70	0.960	1.71	1.70	0.855
Group cycle number	5.8	6.3	0.139	6.3	5.8	0.160
Client age	40	40	0.778	40	40	0.855
Number of children	3	3	0.937	3	3	0.569
Number of economic dependents	2	2	0.830	2	2	0.569
Marital status: married (%)	55	56	0.297	56	55	0.800
Marital status: unmarried partner (%)	19	17	0.051	18	18	0.629
Marital status: divorced (%)	3	3	0.769	3	3	0.887
Marital status: single (%)	19	19	0.894	19	19	0.803
Marital status: widow (%)	4	5	0.071	5	4	0.361
Panel B. Borrower-level indicators						
Take-up of additional modules (%)	70.0	67.9	0.037	69.8	68.3	0.116
Number of modules of coverage	1.74	1.74	0.900	1.75	1.74	0.242
Clients' loan cycle number	5.6	5.7	0.311	5.6	5.7	0.246
Client age	40	40	0.408	40	40	0.533
Number of children	3	3	0.680	3	3	0.190
Number of economic dependents	2	2	0.475	2	2	0.472
Marital status: married (%)	55	57	0.045	56	56	0.584
Marital status: unmarried partner (%)	18	17	0.122	17	17	0.808
Marital status: divorced (%)	3	3	0.828	3	3	0.559
Marital status: single (%)	20	19	0.214	19	19	0.768
Marital status: widow (%)	4	5	0.575	5	4	0.472

Group-level indicators are averages of the group-average of each indicator (e.g. the average of the percentage of clients within a group who purchase additional insurance modules). Client-level indicators report direct averages without taking into account the group structure of the data. All data are for borrowers included in the randomization, from the last loan cycle before the interventions.

Panel A reports the average of group-level indicators. The average take-up, for example, is the group-level average of the percentage of clients in a group who purchase any additional module of insurance. The randomizations for both the subsidy and the information treatments were overall successful in creating similar groups in terms of insurance purchasing behavior and key demographic characteristics. The percentage of clients living with an unmarried partner is slightly lower in groups assigned to not receive the subsidy than in groups assigned to keep the subsidy, but the absolute value of the difference is small (2 percentage points) and combining this marital status category with married clients shows no statistically significant difference. Groups assigned to the financial or emotional information were remarkably similar.

Randomizing groups into treatments does not guarantee, however, that the characteristics of borrowers within these groups will be similar across treatments. In fact, borrowers assigned to the no-subsidy group were statistically significantly less likely to have purchased additional insurance in the last cycle before the interventions were implemented: 68 percent of them did, versus 70 percent of clients assigned to keep benefitting from the subsidy. Given that the analyses are run at the client level, a binary variable indicating whether clients purchased any module of additional insurance in the cycle before the interventions is added to some specifications to control for this difference. In addition to the difference in take-up, the percentage of clients living in an unmarried partnership is also higher for clients assigned to the no-subsidy group. The difference is small and unlikely to impact results, so I do not control for marital status in the regressions. Clients assigned to the financial or emotional information presented very similar characteristics.

4.3.2. Attrition

Attrition stemmed from the fact that not all borrowers included in the randomization took a new loan immediately following the end of their group's cycle. About 30 percent of randomized borrowers did not renew their loan, a group that is statistically significantly different from no-attriters, although treatment status was not associated with whether a borrower attrited.

Power calculations indicated that a sample of 545 groups of borrowers was necessary to detect a small effect size in the sense of Cohen (1988). The large sample size was mainly due to the homogeneity of insurance purchase decisions within groups, which translated into a high intra-cluster correlation coefficient. A total of 826 groups of borrowers were randomly assigned to the treatments, and 567 actually started a new loan cycle during the intervention period. Borrowers in these groups constitute the analysis sample.

Since analyses are conducted at the client level, I calculate attrition in terms of borrowers and not groups of borrowers. The 826 randomized groups included 12,486 borrowers immediately before the intervention. Of these, 8,763 started a new loan cycle with Compartamos and had an opportunity to purchase insurance under experimental conditions. The other 3,723 chose not to take a new loan, either leaving Compartamos definitively or taking a pause between two loan cycles. This rate of attrition of nearly 30 percent is high but not uncommon in microfinance in general, and in Compartamos in particular (as confirmed informally by managers). Karlan and Valdivia (2011), for example, report a turnover rate around 60 percent for clients of a microfinance institution lending with a similar methodology in Peru.

Attrition is a problem if it is not orthogonal to the treatments (Duflo, Glennerster, & Kremer, 2007). Table 3 presents the coefficients from a regression of a binary variable equal to one if a borrower was included in the randomization but did not borrow in the following loan cycle, and zero otherwise. The data is pre-intervention, for the 12,486 borrowers included in the randomization. The results indicate that attrition is not random: borrowers who were younger or single, had more children, had a shorter credit history with Compartamos, took out smaller loans and did not purchase additional insurance were more likely to drop out. Attrition also significantly varied by branch. The good news, however, is that treatment assignments were uncorrelated with the likelihood that a client dropped out of the sample.

Table 3. Characteristics of attrition.

Dependent variable: dummy = 1 if client was randomized but did not borrow post-interventions	Subsidy treatment	Information treatment	Both treatments
Treatment: 1 if no subsidy; 0 if subsidy (i.e. no change)	0.020		0.021
	(0.018)		(0.018)
Treatment: 1 if financial information; 0 if emotional information		0.011	0.012
		(0.018)	(0.018)
Client loan cycle	-0.008***	-0.008***	-0.008***
	(0.002)	(0.002)	(0.002)
Group loan cycle	-0.008***	-0.008***	-0.008***
	(0.002)	(0.002)	(0.002)
Loan amount (in 100s of Pesos)	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Branch: Hermosillo	-0.004	-0.004	-0.004
	(0.025)	(0.025)	(0.025)
Branch: Hermosillo Norte	-0.086***	-0.085***	-0.086***
	(0.030)	(0.029)	(0.030)
Branch: Navojoa	0.122***	0.122***	0.122***
	(0.029)	(0.029)	(0.029)
Branch: Ciudad Obregón	-0.014	-0.014	-0.014
	(0.030)	(0.031)	(0.030)
Client purchased any module of insurance	-0.032**	-0.033**	-0.032**
	(0.015)	(0.015)	(0.015)
Client age	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)
Marital status: Unmarried partner	0.025**	0.024*	0.025**
	(0.013)	(0.013)	(0.013)
Marital status: Divorced	0.010	0.010	0.010
	(0.025)	(0.025)	(0.025)
Marital status: Single	0.070***	0.069***	0.070***
	(0.014)	(0.014)	(0.014)
Marital status: Widow	0.021	0.021	0.020
	(0.020)	(0.020)	(0.020)
Number of children	0.008**	0.008**	0.008**
	(0.004)	(0.004)	(0.004)
Education: Pre-school	-0.056	-0.057	-0.058
	(0.040)	(0.040)	(0.040)
Education: Primary	-0.014	-0.015	-0.015
	(0.037)	(0.037)	(0.037)
Education: Secondary	-0.040	-0.040	-0.041
	(0.038)	(0.038)	(0.038)
Education: Technical	-0.049	-0.050	-0.050
	(0.040)	(0.040)	(0.040)
Education: Professional	-0.053	-0.054	-0.054
	(0.041)	(0.042)	(0.041)
Client owns her home, mortgage fully paid	-0.027**	-0.026**	-0.027**
	(0.012)	(0.012)	(0.012)
Constant	0.594***	0.598***	0.589***
	(0.050)	(0.050)	(0.051)
Observations	11,447	11,447	11,447
R-squared	0.078	0.077	0.078

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses are clustered at the group level. The omitted branch is "Guaymas", the omitted marital status is "married" and the omitted education category is "no formal education."

4.3.1. *Group switch*

Group switches happen when a borrower changes group from one loan cycle to the next. Compartamos does not prohibit or discourage these switches, which happened during the experiment. These switches threaten the integrity of the random assignment when a borrower switches from a group assigned to one treatment to a group assigned to another treatment. In effect, a borrower could have made a decision whether to purchase insurance based on the information that the subsidy would not be eliminated, but actually benefit from it in her next cycle in her new group.

To address the issue of group switches, all analyses are performed on an intent-to-treat basis. Borrowers are considered part of the treatment group to which they were assigned, not the one that they actually received.

In practice, the data that I present in this sub-section show that the switching problem did not impact the result. First, only 356 of the 8,763 borrowers included in the analysis (4 percent) changed group between the cycle in which they received the treatment and the cycle in which I measure insurance purchases. Only 52 of these 356 borrowers switched from one randomized group to another, the others switching to a group that was not included in the experiment. Of the 52, 28 borrowers switched from a group assigned to keep the subsidy to one assigned not to and 24 switched from a group assigned not to keep the subsidy to a group assigned to keep it. The fact that more than half of borrowers who switched from one randomized group to another made a switch that is not in their favor suggest that, overall, switching was unrelated to the treatment assignment.

Table 4 also presents several regressions testing the influence of switchers on the impact of eliminating the subsidy. Column (1) confirms that switching is not statistically significantly related to the treatment assignment: the coefficient on the treatment dummy is small in absolute value (-0.011) and not statistically significant. Columns (2) and (3) implement the main impact specification, with two modifications. The model in column (2) uses a treatment-on-the-treated (TOT) measure of impact, where the treatment dummy variable is determined by the borrower's actual treatment received. In other words, the binary variable is set to 1 if the borrower took a loan post-experiment in a group assigned to not receive the subsidy, even if she was in a different group pre-experiment and was randomly assigned to another treatment. The TOT impact measure is very similar to the main result (Table 7), suggesting that switchers did not impact the overall results. Finally, the model in column (3) is based on an intent-to-treat (ITT) definition of the treatment but includes a binary variable indicating whether the borrower was one of the 356 who switched groups. Here again, the measure of impact is very similar to the preferred estimate presented in Table 7. All in all, borrowers who switch groups did not influence the estimates of the impact of eliminating the subsidy.

Table 4. Impact of group switching on the subsidy results.

Dependent variable:	1 if client switched group		1 if client is covered by any module of insurance	
	(1)	(2)	(3)	(4)
Subsidy treatment (1 = no subsidy; ITT)	-0.011			
	(0.011)			
Information treatment (1 = financial information; ITT)		-0.015		
		(0.011)		
Subsidy treatment (TOT) * Post			-0.102***	
			(0.011)	
Post			0.001	0.001
			(0.008)	(0.008)
Treatment (TOT)			0.009	
			(0.016)	
Subsidy treatment (ITT) * Post				-0.105***
				(0.011)
Treatment (ITT)				-0.417***
				(0.068)
1 if client switched group				-0.163***
				(0.031)
1 if bought insurance in "pre" wave			0.008**	-0.417***
			(0.004)	(0.068)
Constant	-0.000	-0.000	0.132	1.195***
	(.)	(0.000)	0.008**	(0.044)
			(0.004)	
Observations	8,763	8,763	1.046***	17,526
R-squared	0.101	0.102	(0.036)	0.161

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the village bank level in parentheses. Columns (1) and (2) report the coefficients from cross-sectional regressions of the treatment indicators on a binary variable equal to 1 if the borrower switched group in the post-experiment loan cycle and 0 otherwise, with pre-experiment data. Columns (3) and (4) report coefficients from panel regressions including village banks, loan officer and time fixed effects. Panel regressions for the information treatment are not shown because the treatment had no average impact. The “pre” wave is the last loan cycle before the experiment. The treatment on the treated (TOT) indicator was set according to the actual treatment received (i.e. based on the borrower’s group in the “post” wave). The intent-to-treat (ITT) indicator was set according to the result of the randomization (i.e. based on the borrower’s group in the “pre” wave), regardless of the actual treatment received.

4.3.2. Implementation

A poor implementation could pose a threat to the experiment’s internal validity. Although all analyses were conducted on an intent-to-treat basis, problems in the implementation could reduce and/or bias the results if loan officers systematically did not implement the treatments assigned to each group of borrowers. This does not appear to have been the case.

During the implementation of the project, a team of auditors was tasked with observing loan officers in randomly-selected groups at the time when they presented the posters and delivered the news that the subsidy would be eliminated (in relevant groups). Auditors observed a total of 143 groups, slightly more than a quarter of all groups involved in the experiment. During these observations, auditors observed three groups (about 2 percent of monitoring observations) in which the subsidy assignment was not respected: in two groups assigned to keeping the subsidy the loan officer announced that the subsidy would be eliminated, and in one group the loan officer forgot to inform clients that they would not enjoy the benefit of the subsidy in their next loan cycle. Non-compliance with the information treatment was also observed in three groups: in three groups assigned to the emotional information the loan officer presented the financial poster. All in all, non-compliance involved six groups and six loan officers. I do not have data on compliance other than from auditor observations, which do not guarantee compliance in the absence of observations, but these monitoring data suggest that no systematic bias was introduced at the implementation stage.

4.3.3. Spillovers

Spillovers, or the contamination of village banks with the treatment intended for other groups, are not a particular concern in the subsidy treatment. Clients can only purchase insurance against their own death, so the benefit of a subsidy assignment cannot spill over to borrowers in the no-subsidy treatment.

The information treatment, on the other hand, can be subject to contamination if clients who were presented with one message discussed the message with clients in a group in the other information treatment. Compartamos typically manages several groups per neighborhood, reinforcing the possibility that such client proximity happen.

To get an understanding of whether spillovers happened, I rely again on monitoring data. During their attendance in a borrowing group meeting, auditors made note of whether they observed or heard any indication that clients were aware of other groups receiving a different message regarding the availability of the free insurance module and/or were presented a different poster. In only one of the 143 groups observed – a group assigned to keep the benefit of the subsidy – did an auditor indicate that clients referred to other groups not having that free module. No auditor observed any client in any group making any reference to other clients being presented different information from the one they received. All in all, available data suggest that contamination was not a problem in this experiment.

5. Results

5.1. Descriptive statistics

In this sub-section, I describe the sample of borrowers who participated in the intervention and compare the average take-up under each treatment. Table 5 provides descriptive statistics on borrowers who participated in the project, at the last loan cycle before the interventions were implemented. It shows that borrowers are middle-aged, literate mothers (less than one percent have no children), with a two-year relationship with Compartamos on average. About half of all clients purchase additional insurance modules, but virtually all buyers only purchase one module.

Table 5. Descriptive statistics on borrowers in the sample.

Age	40 (12)
Education level (%)	
No formal education	2 (13)
Pre-school	19 (39)
Primary	19 (39)
Secondary	44 (50)
Technical	9 (28)
Professional	9 (28)
Marital status (%)	
Married	56 (50)
Unmarried partner	17 (38)
Divorced	3 (17)
Single	19 (39)
Widow	4 (21)
Number of children	3.0 (1.6)
Number of economic dependents	2.1 91.1)
Borrower owns her home, mortgage fully paid (%)	70 (46)
Borrower loan cycle number	5.7 (4.0)
Loan size (Mexican Pesos)	8,808 (6,500)
Loan size (US Dollars)	691 (510)
Group cycle number	6.7 (4.8)
Purchased any additional insurance (%)	69 (46)
Purchased module 1 (%)	66.3
Purchased module 2 (%)	1.89
Purchased module 3 (%)	0.29
Purchased module 4 (%)	0.09
Purchased module 5 (%)	0.02
Purchased module 6 (%)	0.03
Purchased module 7 (%)	0.39

Notes: Data are averages and standard deviations in parentheses for 8,763 borrowers who participated in the experiment. Additional coverage is purchased in modules of 15,000 Mexican Pesos (about US$1,175 at the time of the experiment).

Since borrowers were randomly assigned to the treatments, comparing the means of the outcomes provides an unbiased estimate of impacts. Although I use a regression framework below to control for the difference in purchasing behavior pre-intervention, the direct comparison of average insurance take-up presented in Table 6 shows the main effects: the average rate and amount of insurance coverage decreased when the subsidy was eliminated, and the information treatment did not impact average coverage.

Table 6. Main impacts of the treatments on insurance coverage.

	% borrowers with any coverage			# modules of insurance coverage		
Panel A. Subsidy treatment	Subsidy	No subsidy	p-value	Subsidy	No subsidy	p-value
Pre-intervention	100	100	1.000	1.7	1.7	0.900
Post-intervention	100	86.5	<0.001	2.0	1.2	<0.001
Change pre to post	*0*	*-13.5*	*<0.001*	*+0.29*	*-0.56*	*<0.001*
Panel B. Information treatment	Emotional	Financial	p-value	Emotional	Financial	p-value
Pre-intervention	100.0	100.0	1.000	1.8	1.7	0.242
Post-intervention	93.1	93.7	0.279	1.6	1.6	0.066
Change pre to post	*-6.9*	*-6.3*	*0.279*	*-0.11*	*-0.13*	*0.295*

5.2. Eliminating the subsidy decreases coverage

Regression analyses in a difference-in-difference framework confirm the results from Table 6. When controlling for borrowers' insurance purchase behavior pre-intervention and group, loan officer and time effects, eliminating the free-module subsidy caused an 11 percentage point drop in the likelihood of having any coverage, and a 0.86-module drop in the amount of coverage, on average (Table 7).

The drop is not identical for borrowers of all age groups, however. Younger borrowers (aged 17 to 29 years) exhibited a larger drop in their likelihood to be covered than older borrowers when the price of coverage increased following the elimination of the subsidy (Table 7, column (2)). The percentage of borrowers who had some insurance coverage dropped by 16.4 points for those aged 17 to 29, against 12.8 and 12.3 points for those aged 30-49 and 50-89, respectively. Point estimates from column (4) in Table 7 also suggest that the average amount of coverage dropped less for older borrowers than for younger and middle-aged borrowers, but the coefficients are not statistically significant.

Table 7. Impact of eliminating the subsidy on insurance coverage.

Dependent variable:	1 if client has any insurance coverage; 0 otherwise		Number of modules of insurance coverage (0-8)	
	(1)	(2)	(3)	(4)
Treatment (1=no subsidy) * Post	-0.105***	-0.130***	-0.857***	-0.864***
	(0.011)	(0.015)	(0.049)	(0.052)
Post	0.003	0.001	0.355***	0.363***
	(0.008)	(0.008)	(0.053)	(0.050)
Treatment	-0.394***	-0.394***	0.036	0.037
	(0.101)	(0.100)	(0.298)	(0.296)
1 if bought insurance in "pre" wave	0.008**	0.007**	0.629***	0.626***
	(0.004)	(0.004)	(0.019)	(0.019)
Treatment * Post * Age group 30-49 years		0.033**		-0.010
		(0.013)		(0.050)
Treatment * Post * Age group 50-89 years		0.027*		0.055
		(0.016)		(0.068)
Constant	1.227***	1.276***	1.293***	1.507***
	(0.053)	(0.055)	(0.179)	(0.175)
Observations	17,526	17,526	17,526	17,526
R-squared	0.151	0.154	0.208	0.210

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the village bank level in parentheses. The coefficients for age groups, the interaction post*age groups interaction, and the treatment*age group interaction are included in the regressions but not shown for clarity purposes. All regressions include village banks, loan officer and time fixed effects. The omitted age group is 17-29 years old. The "pre" wave is the last loan cycle before the experiment.

5.3. Information does not impact coverage overall

Table 8 shows that neither message had any impact, on average, on insurance coverage. While the likelihood to be covered by any amount of insurance dropped for all borrowers post-interventions, as a consequence of the elimination of the subsidy in some village banks, the interaction term showing the impact of the financial information versus that of the emotional information is very small and not statistically significant. Point estimates suggest that the emotional information may have limited the drop in coverage, but the coefficients in column (1) are not statistically significant. Coefficients in column (3) suggest that the emotional message is better than the financial message at mitigating the drop in the amount of coverage, but this impact is not statistically different from zero.

The impact of information is strongly dependent on borrowers' age, however, even more so than the impact of eliminating the subsidy. Each age group reacted differently, but two main findings emerge from analyzing the triple-interaction coefficients in columns (2) and (4) of Table 8. First, younger borrowers were more influenced by the emotional information than by the financial information. Borrowers aged 17 to 29 who were exposed to the emotional poster and script were 4.3 percentage points more likely to carry any coverage, and they carried 0.13 more modules of coverage, on average, than their counterparts exposed to the financial poster. Second, middle-aged borrowers reacted in an opposite fashion: the financial information led them to *increase* their likelihood to be covered by 2 percentage points, and limited the drop in the amount of coverage by 0.12 modules (for a net drop of 0.01 modules from the baseline level). As for older borrowers, aged 50 and above, the two different messages did not statistically significantly influence their coverage.

Table 8. Impact of information on insurance coverage.

Dependent variable:	1 if client has any insurance coverage; 0 otherwise		Number of modules of insurance coverage (0-8)	
	(1)	(2)	(3)	(4)
Treatment (1=financial information) * Post	0.001	-0.043***	-0.057	-0.132**
	(0.011)	(0.016)	(0.060)	(0.067)
Post	-0.046***	-0.038***	-0.005	0.034
	(0.011)	(0.013)	(0.065)	(0.067)
Treatment	0.079	0.087	0.207	0.222
	(0.143)	(0.143)	(0.312)	(0.313)
1 if bought insurance in "pre" wave	0.008**	0.008**	0.629***	0.626***
	(0.004)	(0.004)	(0.019)	(0.019)
Treatment * Post * Age group 30-49 years		0.063***		0.121**
		(0.014)		(0.060)
Treatment * Post * Age group 50-89 years		0.039**		0.032
		(0.016)		(0.078)
Constant	0.948***	0.958***	1.307***	1.237***
	(0.082)	(0.082)	(0.206)	(0.206)
Observations	17,526	17,526	17,526	17,526
R-squared	0.106	0.110	0.131	0.133

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the village bank level in parentheses. The coefficients for age groups, the interaction post*age groups interaction, and the treatment*age group interaction are included in the regressions but not shown for clarity purposes. All regressions include village banks, loan officer and time fixed effects. The omitted age group is 17-29 years old. The "pre" wave is the last loan cycle before the experiment.

5.4. Information mitigates the impact of eliminating the subsidy on the likelihood to have coverage, for some age groups

A more complex picture emerges when looking at the impact of eliminating the subsidy by information treatment, and for different age groups. As reported above, the information treatment did not impact insurance coverage overall. The information treatment also did not interact statistically significantly, on average, with the subsidy treatment: the drops in likelihood to be covered and amount of coverage were statistically similar for borrowers in each of the four groups created by the two treatments. Simple averages suggest that the financial information limited the drop in coverage caused by eliminating the subsidy (data under heading "All ages" in Figure 1 and Figure 2), but this difference is not statistically significant (regression coefficients not shown).

Figure 1. Average impact of eliminating the subsidy on the likelihood to be covered, by information treatment and age.

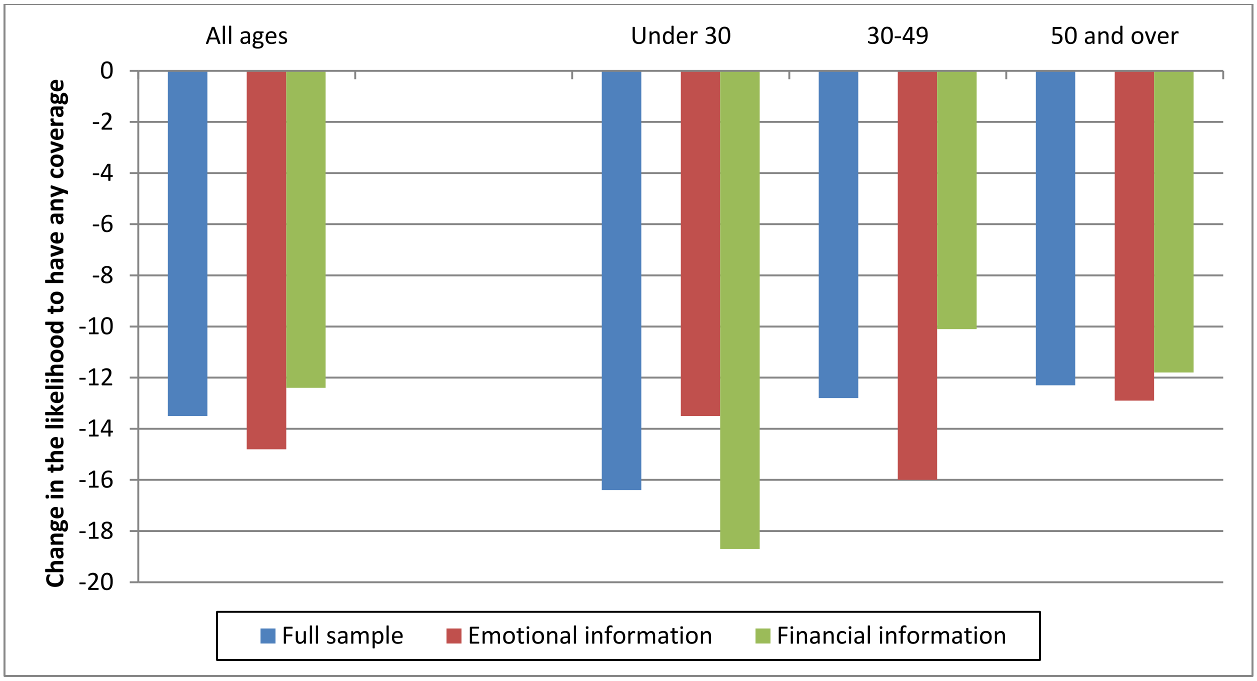
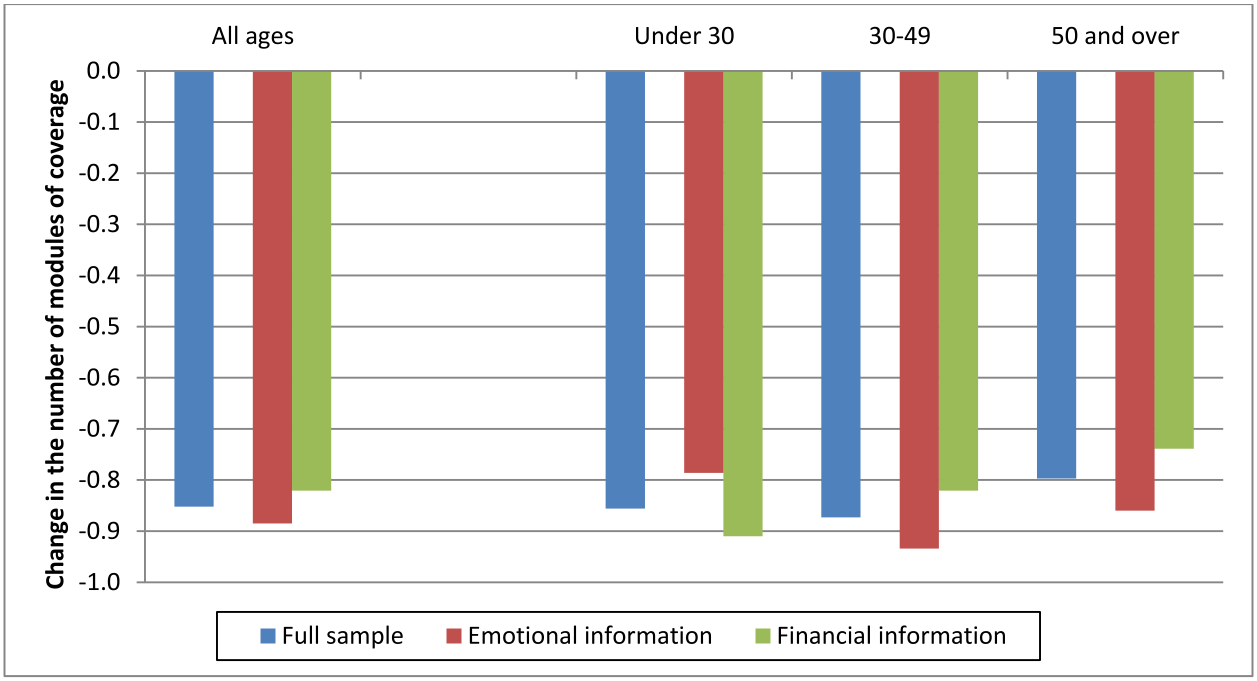


Figure 2. Average impact of eliminating the subsidy on the amount of coverage, by information treatment and age.



This lack of average impact hides important differences by age group, however, as shown visually in Figure 1 and Figure 2, and confirmed statistically in Table 9. The main difference is in the impact of the treatments – and their interaction with age – on the likelihood to have any coverage, for younger and middle-aged borrowers. For borrowers 50 years and older, the impact of eliminating the subsidy, on both extensive and intensive margins, was statistically identical whether they received the financial or emotional information.

For younger borrowers, the emotional poster limited the drop in the likelihood to be covered brought about by the elimination of the subsidy: on average, eliminating the subsidy caused borrowers aged 17 to 29 years to decrease their likelihood to have any coverage by 13.5 percentage points for those presented the emotional information, against 18.7 percentage points for those presented the financial information. This difference is statistically significant at the 95 percent confidence level (Table 9). On the intensive margin, the emotional information may have limited the drop in the number of modules of coverage that young borrowers had, although that difference is not statistically significant.

For middle-aged borrowers, the results are opposite: when the subsidy was eliminated, the financial information caused a lower drop in coverage in this age group. Middle-aged borrowers who were given financial information were 10.1 percentage points less likely to have coverage when the subsidy was eliminated, against 16 percentage points less likely to have coverage when the subsidy was eliminated if they were presented the emotional information. This difference is statistically significant, and statistically significantly different from the impact on younger borrowers (Table 9).

Table 9. Select regression estimates of the impact of eliminating the subsidy, by information treatment and age groups.

Dependent variable:	1 if client has any insurance coverage; 0 otherwise	Number of modules of insurance coverage (0-8)
Subsidy treatment * Information treatment * Post	-0.063**	-0.108
	(0.029)	(0.112)
Subsidy treatment * Information treatment * Post * 30-49 age group	0.109***	0.210**
	(0.026)	(0.102)
Subsidy treatment * Information treatment * Post * 50-89 age group	0.066**	0.205
	(0.031)	(0.139)
Constant	1.151***	1.153***
	(0.074)	(0.262)
Observations	17,526	17,526
R-squared	0.160	0.211

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the village bank level in parentheses. All regressions include village banks, loan officer and time fixed effects. Only coefficients for the triple interaction terms are shown for clarity; the regressions include all combinations of the three variables as well as a binary indicator of whether the borrower purchased insurance in the "pre" wave. Full results are presented in Appendix 3. The "pre" wave is the last loan cycle before the experiment.

5.5. <u>Robustness checks</u>

In this section I focus on two tests of the robustness of the findings.

5.5.1. 5-period analysis

The first robustness test uses additional data. Instead of a 2-period panel dataset using data from one loan cycle before the experiment and one after the experiment, I take advantage of additional data on all active borrowers in 2011. These data include information on insurance coverage for up to five loan cycles in total (four of them before the experiment and one after).

The results are qualitatively similar to the 2-period results presented in Table 7 and Table 8. The main difference is that the coefficients indicating the impact of eliminating the subsidy are larger in absolute value: the coefficients in Table 10 suggest that eliminating the subsidy led to a decrease in coverage by 11.7 percentage points (instead of 10.5 points in the 2-period analysis) and a decrease in the average number of modules of insurance coverage by 0.87 modules (instead of 0.86 modules in the 2-period analysis). The magnitude of the average impacts of the financial information on coverage, and levels of statistical significance for the coefficients measuring the impact of both treatments, are similar in the 2-period or 5-period analyses. Overall, these results do not alter the main conclusions.

Table 10. Impact of the treatments on insurance coverage, 5-period panel data.

Dependent variable:	1 if client has any insurance coverage; 0 otherwise		Number of modules of insurance coverage (0-8)	
	(1)	(2)	(3)	(4)
Panel A. Subsidy treatment				
Treatment (1=no subsidy) * Post	-0.117***	-0.141***	-0.864***	-0.859***
	(0.011)	(0.015)	(0.045)	(0.045)
Post	-0.000	-0.002	0.367***	0.387***
	(0.004)	(0.003)	(0.039)	(0.035)
Treatment	-0.046***	-0.047***	-0.012	-0.015
	(0.014)	(0.014)	(0.088)	(0.086)
1 if bought insurance in "pre" wave	0.007***	0.006***	0.427***	0.423***
	(0.002)	(0.002)	(0.016)	(0.016)
Treatment * Post * Age group 30-49 years		0.031**		-0.027
		(0.013)		(0.045)
Treatment * Post * Age group 50-89 years		0.031**		0.047
		(0.015)		(0.063)
Constant	0.954***	0.954***	1.373***	1.309***
	(0.031)	(0.031)	(0.166)	(0.167)
Observations	35,107	35,107	35,107	35,107
R-squared	0.131	0.133	0.138	0.140
Panel B. Information treatment				
Treatment (1=financial information) * Post	0.007	-0.035**	-0.010	-0.105*
	(0.012)	(0.016)	(0.057)	(0.060)
Post	-0.062***	-0.054***	-0.055	0.011
	(0.009)	(0.010)	(0.049)	(0.052)
Treatment	0.008	0.012	-0.027	-0.011
	(0.014)	(0.014)	(0.070)	(0.071)
1 if bought insurance in "pre" wave	0.006***	0.006**	0.427***	0.422***
	(0.002)	(0.002)	(0.016)	(0.016)
Treatment * Post * Age group 30-49 years		0.061***		0.133**
		(0.014)		(0.054)
Treatment * Post * Age group 50-89 years		0.037**		0.086
		(0.016)		(0.071)
Constant	0.941***	0.942***	1.464***	1.385***
	(0.035)	(0.029)	(0.170)	(0.171)
Observations	35,107	35,107	35,107	35,107
R-squared	0.083	0.086	0.074	0.077

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the village bank level in parentheses. All regressions include village banks, loan officer and time fixed effects. The “pre” wave is the last loan cycle before the experiment.

5.5.2. Test of the experimental design on non-experimental data

I take further advantage of the 5-period data by analyzing a "fake experiment." I run the analysis on the four waves of data pre-experiment. These data cover the 8,763 clients who will later participate in the experiment, but provide information on previous loan cycles. The analysis is identical to that in Table 7 and Table 8, in that it considers only two periods of data for each regression. All borrowers had automatic insurance coverage through the subsidized module in periods T1 to T4, so I focus on the number of modules of insurance coverage.

Given that no manipulation of the subsidy was implemented in these cycles, the interaction of the binary variable indicating that a client was in a group that is later assigned to not receive the subsidy and a binary variable indicating the "post" loan cycle should be close to zero and not statistically significant. Similarly, no change to the marketing approach and message was implemented, so the information treatment should not have impacted coverage. The (treatment * post) coefficients in Panels A and B of Table 11 show that this was the case.

Table 11. Test of the experimental design on non-experimental data.

Period of data:	T1 to T2	T2 to T3	T3 to T4
Dependent variable:	Number of modules of insurance coverage (0-8)		
Panel A. Subsidy treatment			
Treatment (1=no subsidy) * Post	-0.016	0.020	-0.010
	(0.040)	(0.050)	(0.039)
Post	-0.056*	-0.015	0.273**
	(0.033)	(0.041)	(0.128)
Treatment	-0.041	0.078	0.179
	(0.060)	(0.092)	(0.155)
Constant	2.431***	1.365***	0.543
	(0.136)	(0.180)	(0.364)
Observations	10,378	11,308	15,966
R-squared	0.067	0.039	0.023
Panel B. Information treatment			
Treatment (1=financial information) * Post	-0.002	0.024	0.028
	(0.041)	(0.049)	(0.040)
Post	-0.063*	-0.019	0.241*
	(0.032)	(0.041)	(0.129)
Treatment	0.078	-0.195**	-0.205**
	(0.052)	(0.086)	(0.098)
Constant	2.389***	1.500***	0.824**
	(0.124)	(0.179)	(0.367)
Observations	10,378	11,308	15,966
R-squared	0.068	0.040	0.023

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the village bank level in parentheses. All regressions include village banks, loan officer and time fixed effects. Data are at the borrower-loan cycle level, for active borrowers between 2011/01/01 and 2012/04/30. The dataset includes up to 5 loan cycles (T1 to T5) per borrower; the experiment was implemented at the end of T4, with take-up observed in T5. The treatment variable was created as intent-to-treat, i.e. it is set to 1 if the borrower was in a group assigned to not receive the subsidy in T5 (subsidy treatment) or to be informed with the financial message in T5 (information treatment).

6. Conclusion

The behavior of borrowers in response to a price change induced by the elimination of a subsidy indicates that potential microinsurance clients are sensitive to the price of microinsurance products. As the subsidy was eliminated and the price doubled for almost all potential clients, the average likelihood of having any coverage dropped by 11 percentage points, and the amount of coverage dropped by 0.86 modules of insurance.

Several factors influenced that sensitivity, however. The negative impact of eliminating the subsidy on total insurance coverage was mitigated by the information provided to clients about the product and its benefits, and depended on the age of the borrowers. Presenting emotional information about the benefits of the insurance helped younger borrowers compensate for the loss of the subsidy and led to higher percentage of borrowers having some coverage. On the contrary, financial information about how the insurance payout helps pay funeral costs led middle-aged borrowers to be more likely to have some coverage. Borrowers aged 50 or older reduced their coverage when the subsidy was eliminated, albeit not as much as those younger than 50, but no specific information impacted this reduction.

The information provided, however, only impacted borrowers' decision to have insurance on the extensive margin. When the subsidy was eliminated and with specific information, borrowers were more likely to decide to purchase one module, to maintain a basic coverage, but were not likely to purchase more modules to maintain – much less increase – their previous level of coverage.

Helping borrowers size the risks they face, even when they are aware of the existence of the risk itself, led to a lower drop in insurance coverage in the face of the elimination of the subsidy. As information can help households make better health decisions, specific information provided at the time of choice was critical to help borrowers make a decision regarding a risk management strategy, and should be part of any intervention aiming to improve the lives of poor households in developing countries.

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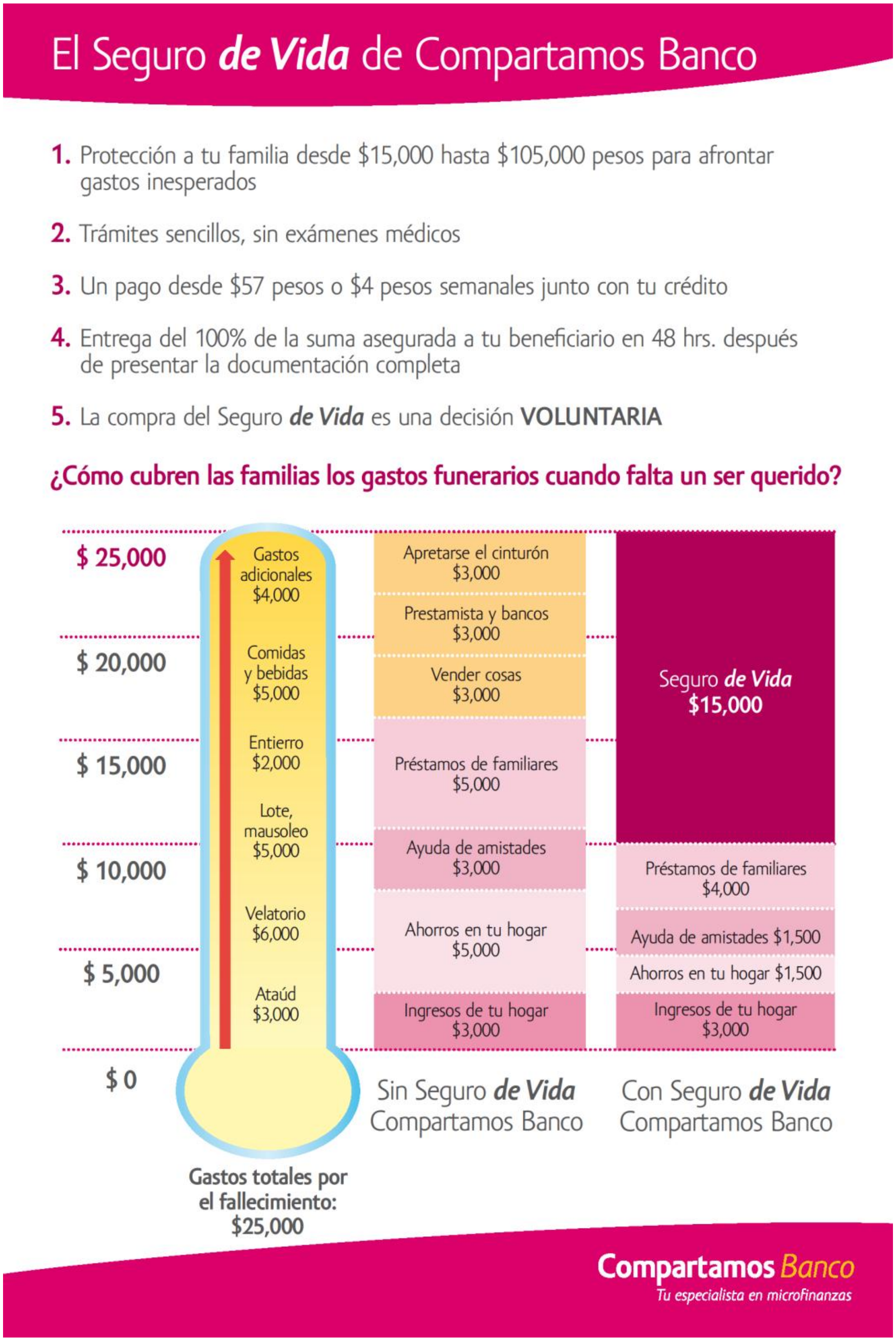
Appendix 1. Poster used to deliver the financial information.



Appendix 2. Poster used to deliver the emotional information.

Appendix 3. Full results of the impact of eliminating the subsidy, by information treatment and age groups.

Dependent variable:	1 if client has any insurance coverage; 0 otherwise	Number of modules of insurance coverage (0-8)
1 if bought insurance in "pre" wave	0.007**	0.626***
	(0.003)	(0.019)
Subsidy treatment (1=no subsidy)	-0.505***	-0.004
	(0.138)	(0.398)
Information treatment (1=financial information)	-0.026	0.143
	(0.114)	(0.357)
Subsidy treatment * Information treatment	0.264	0.102
	(0.239)	(0.554)
Post	0.002	0.362***
	(0.009)	(0.062)
Subsidy treatment * Post	-0.094***	-0.803***
	(0.018)	(0.085)
Information treatment * Post	-0.003	0.001
	(0.005)	(0.070)
Subsidy treatment * Information treatment * Post	-0.063**	-0.108
	(0.029)	(0.112)
1 if borrower is 30-49 years old	-0.000	0.008
	(0.002)	(0.031)
1 if borrower is 50-89 years old	-0.004*	0.056
	(0.002)	(0.041)
Subsidy treatment * 30-49 age group	0.010*	0.046
	(0.006)	(0.039)
Subsidy treatment * 50-89 age group	0.013	-0.019
	(0.008)	(0.049)
Information treatment * 30-49 age group	-0.001	0.026
	(0.002)	(0.038)
Information treatment * 50-89 age group	0.003	0.020
	(0.002)	(0.050)
Subsidy treatment * Information treatment * 30-49 age group	-0.018**	-0.048
	(0.008)	(0.053)
Subsidy treatment * Information treatment * 50-89 age group	-0.014	0.029
	(0.010)	(0.073)
Post * 30-49 age group	-0.001	-0.007
	(0.003)	(0.049)
Post * 50-89 age group	0.003	0.061
	(0.005)	(0.071)
Subsidy treatment * Post * 30-49 age group	-0.027	-0.125*
	(0.017)	(0.076)
Subsidy treatment * Post * 50-89 age group	-0.010	-0.058
	(0.022)	(0.106)
Information treatment * Post * 30-49 age group	0.007	-0.007
	(0.005)	(0.068)
Information treatment * Post * 50-89 age group	0.002	-0.135
	(0.005)	(0.092)
Subsidy treatment * Information treatment * Post * 30-49 age group	0.109***	0.210**
	(0.026)	(0.102)
Subsidy treatment * Information treatment * Post * 50-89 age group	0.066**	0.205
	(0.031)	(0.139)
Constant	1.151***	1.153***
	(0.074)	(0.262)
Observations	17,526	17,526
R-squared	0.160	0.211

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the village bank level in parentheses. All regressions include village banks, loan officer and time fixed effects. The “pre” wave is the last loan cycle before the experiment.